

Interactive comment on “Microbial bioavailability regulates organic matter preservation in marine sediments” by K. A. Koho et al.

Anonymous Referee #2

Received and published: 5 November 2012

This manuscript by Koho et al presents descriptive as well as experimental data from field work in the Arabian Sea investigating the drivers and constraints of OM cycling under oxygen limitation. Results confirm previous findings on the quality and quantity of organic matter preserved in OMZs, but describe an interesting contrast between measurements of OM degradability and other frequently used methods to describe OM quality. But while the data presented are interesting and deserve publication, I feel the manuscript would benefit from significant revision before publication.

General comments:

1. At the end of the discussion the authors state that the purpose of the paper is to describe OM quantity and quality along a bottom water oxygen gradient, and experimentally test bioavailability and potential microbial remineralisation. While work was

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clearly carried out in a context of investigating the drivers and constraints of OM degradation in marine sediments, I missed a clear statement of the wider relevance of this work, or any specific hypotheses to be tested. Which specific research question was addressed ?

2. The methods section needs some clarification and missing information needs to be added. For example, why was bioavailability tested under identical oxic conditions ? Would it not have been more interesting to run comparative incubations under both standardised and in situ O₂ conditions ? Why were sediments stored under (I believe) anoxic conditions at 4C prior to oxic incubation ? To kill off fauna ? What sediment volume was incubated, and at what oxygen concentration ? As slurries in plastic bags, or bottles ? At some point it appears the incubations were run exactly as in Moodley et al 2011, but even if so, the most important parameters need to be briefly repeated here as not every reader will know the Moodley paper by heart.

Why were the upper 3 sediment cm combined, although pigment and 210Pb profiles clearly confirm the steep gradients to be expected within this sediment layer ? And why were only two replicates incubated, which were then combined for analyses ? The lack of replicates compromises the results so this should be carefully justified.

3. The discussion seems to lack a clear focus and conclusion, and the paragraph on the potential role of macrofauna for OM degradation is somewhat misleading. For example, diversity is not necessarily directly correlated to sediment mixing, burial or feeding rates, and the fact that, as the authors state, ‘the role of fauna in OM processing is thus limited to a few taxa’ is by no means synonymous with it being negligible. This is actually highlighted by the 210Pb and pigment profiles which indicate deep bioturbation at several stations. In addition, the discussion should take into account recent literature on macrofauna – bacteria interactions in OMZ sediments (Hunter et al 2012, ISME Journal). While the present manuscript clearly confirms previous data regarding the quantity and quality of OM preserved in OMZ sediments, the interesting discrepancy found between quality indices and microbial degradability remains almost

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unexplored and a paragraph or two should be dedicated to discuss this and develop ideas of potential underlying causes.

Specific comments:

Method descriptions/ references need to be checked and completed, some are missing (e.g. bioturbation, statistics etc).

Reference list needs to be checked, some publications cited in the text do not appear in reference list.

Fig 1 inset should be enlarged or area of inset reduced to make it easier to identify the location of the Murray Ridge within the northern Arabian Sea.

Fig. 4 will likely be very difficult to read, unless set on a whole page in the final layout

Interactive comment on Biogeosciences Discuss., 9, 13187, 2012.